

prevalence of prairie fires in spring and autumn. Personally I favour the latter, though both causes may in part be answerable. If worms abound in Iceland ( $65^{\circ}$  N. lat.), in Kerguelen Land ( $50^{\circ}$  S. lat.), and in Toronto ( $43^{\circ}4'$  N. lat., mean winter temperature  $27\frac{1}{4}^{\circ}$  F.), why should they not also occur at Winnipeg ( $50^{\circ}$  N. lat.)? Certainly the mean winter temperature is very low, being about  $8^{\circ}$  F., and the mean minimum for eleven years —  $40\frac{3}{4}^{\circ}$  F. I made special inquiries as to the depth to which the soil in Manitoba becomes frozen in winter. This is often as much as five or six feet, but only, I believe, in the more exposed places, and certainly as a rule it is thawed again in the spring. I do not think this would render the ground uninhabitable by worms when they are able to exist in Iceland. Mr. Darwin says nothing as to the effect of frost on worms except (p. 26) that "worms are sensitive to a low temperature, as may be inferred from their not coming out of their burrows during a frost"; but he states (p. 110) that they are easily able to descend three or four or even seven or eight feet below the surface. It would be interesting to ascertain whether worms inhabit equally cold portions of the Old World.

But the agency which I believe has caused the absence of earthworms from the North-West is, as already stated, the prairie fires which annually sweep over enormous portions of the country, totally consuming the grass, and converting it into a black ash. This, it might well be imagined, would for months together completely deprive any worms that formerly existed of that variety of decaying vegetable matter that composes their food; and assuming that fires have annually passed over large portions of the prairies for scores of generations (as seems in every way probable), it appears to me only reasonable to suppose that this cause would effectually have exterminated the worms from the country or have prevented them occupying it. It is my belief (as I shall elsewhere state more fully) that the very fertile, fine, black, powdery, and almost soot-like soil from one to three feet thick, even the open, treeless nature of the prairies themselves, and the absence from their surface, so far as my observation goes, of every single species of mollusk, while many species abound in all the ponds, lakes, and streams, are all in a large degree, if not entirely, due to the action of the fire. If this view ultimately turns out to be correct, it will be further seen that the very means which has deprived the soil of the North-West of that natural cultivation which the soils of most other countries enjoy has, at the same time, liberally supplied it with a manure resulting from the charred ashes of the grass which is annually burned. My friend, Mr. T. Rogers, who has taken much interest in the absence of worms from the North-West, and is inclined to attribute it rather to frost than to fire, though he suggests that the "alkali" may possibly have had something to do with it, has already brought the subject before the Literary and Scientific Society of Manchester, where he seems to have met with a good deal of incredulity.

As another evidence of the absence of worms, the numerous, large, Glacial boulders that strew the prairies around Brandon and elsewhere may be cited. These, had worms existed, would doubtless have long ago been lowered beneath the surface, as also the skulls and other bones of buffaloes, which so abound on the prairies, and most of which have evidently lain there a long while. Nevertheless some of these have been buried in the course of time, as one gentleman told me that he had sometimes turned them up from a depth of two or three inches beneath the surface when ploughing. Their burial may have been accomplished by the wind drifting soil over them, or by the working of gophers. Of these peculiar little animals two species are very abundant on the prairies, where they make extensive burrows, which it seems possible may to some extent accomplish the natural cultivation of the soil in the way worms are accustomed to do it elsewhere. Some more suggestive remarks on this point may be found in a paper by Mr. Seton, published in the Report of the Manitoba Department of Agriculture for 1882, and which may be studied with advantage. ROBT. MILLER CHRISTY  
Chignal St. James, near Chelmsford, December 20, 1883

#### Magnetic Dip in South China and Formosa

WHILE engaged on a meteorological mission in China I availed myself of the opportunity to make the following determinations of the magnetic dip. The observations in Hong Kong were made at the public gardens, the Observatory being not yet

ready. On October 10 I observed at the British Consulate; on November 3 at the English Pre-byterian Missions Compound, Swatow. In Amoy I observed at the residence of the Commissioner I.M. Customs, in Takow (Formosa) at the Custom House, and at the South Cape (Formosa), near the magnificent fortified lighthouse. It is to be feared that the observations on the coast of China are slightly vitiated from local attraction, the rocks consisting of ferruginous granite. Southern Formosa is built up of coral, raised in places to a great height, no doubt through volcanic action. Slight earthquakes are of common occurrence in Formosa, whereas along the coast of China they are rare and of no importance except to the seismologist.

Place	Date	Local M.T.		Dip.
		h.	m.	
Hong Kong ...	1883, Nov. 5 ...	5	9 p.m.	32 17
" ...	" 9 ...	5	4 "	32 19
Swatow ...	Oct. 10 ...	5	24 "	34 23
" ...	Nov. 3 ...	11	25 a.m.	34 17
Amoy ...	Oct. 14 ...	3	50 p.m.	36 45
" ...	" 16 ...	5	10 "	36 50
Takow ...	" 24 ...	2	45 "	32 54
South Cape ...	" 27 ...	4	0 "	31 24
" ...	" 28 ...	4	30 "	31 27.5
" ...	" 29 ...	3	20 "	31 24.5

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#### THE ORIGIN OF CORAL REEFS

REGARDING this interesting geological problem, which has recently been discussed in NATURE, we are enabled through the kindness of Mr. Murray, of the Challenger Commission, to publish a letter which has been addressed to him by Dr. Guppy from the Pacific. The importance of this communication will be recognised in the confirmation it supplies of the inference that coral reefs start upon a platform of limestone composed of the remains of foraminifera, &c., and are themselves of no great thickness. Dr. Guppy will no doubt continue his researches, and we may hope to obtain from him precise data regarding the average thickness of the coral rock, the lithological difference between it and the underlying limestone, the structure of the limestone, whether any succession of organisms can be detected in it, and whether at any point the underlying volcanic rock can be seen which would afford a measurement of the thickness of the calcareous deposits. The effects of denudation and their relation to height above the sea will no doubt also receive his attention.

"Shortlands Islands, Solomon Group,  
August 7, 1883

"During the twelve months I have spent in this group of islands—serving as surgeon on board H.M. surveying-ship *Lark*—I have been much interested in and have devoted considerable attention to the raised coral formations in various islands; and as my observations may be of service towards confirming the views which you have advanced with reference to coral islands and reefs, I will state briefly the results of my observations.

"Excluding the large continental islands, I will refer for the sake of brevity to the numerous small islands of this archipelago, those of volcanic, and those of calcareous formations. Confining myself to the islands of calcareous formation, I will pass over the numerous small islands which are entirely composed of coral detritus, sand, and shells, and have been formed by the materials thrown up by the waves at the present sea-level; and will restrict my remarks to a very common type of islands in this group, with gently sloping and rounded profile, having an elevation varying perhaps between 100 and 1100 or 1200 feet, and composed in bulk of an impure earthy or argillaceous limestone, usually bedded, and almost always foraminiferous, now and then rich in other pelagic organisms, such as *Pteropods*. On this rock rests the

coral limestone, which forms but a comparatively thin crust, and has been altogether removed from most of the higher regions by sub-aërial agencies. However, I have observed the raised coral rock still preserved at considerable heights above the sea, and in two localities at elevations of 900 feet.

"Amongst the sub-group known as the Shortland Islands, I came upon beds of this *impure calcareous rock* (beneath the raised coral rock) *abounding in Pteropods*, mostly *Hyalea*, and large foraminiferous tests, mingled with shells, some of them of shallow water habit.

"I am, &c., "H. B. GUPPY"

#### A FORGOTTEN EVOLUTIONIST

A BOOK has lately come into my hands a few words about which may possibly interest some of the readers of NATURE. Its title is "Histoire Naturelle des Fraisiers"; the author was A. N. Duchesne, and it was published at Paris in 1766. It must be, I suspect, an uncommon book, for there is no copy in the library of the Royal Gardens at Kew. And this library, comprising as it does the contributions of many collectors who allowed little to escape them, is remarkably complete; Mr. Daydon Jackson has in fact found in it more than a thousand publications the titles of which are not to be met with in the last edition of Pritzel's well-known "Thesaurus."

The scarceness of a botanical book is not perhaps in itself a matter of any great moment, and I bought the book out of a provincial sale catalogue without expecting it to be particularly interesting, though I knew Duchesne's name as an authority on the cultivated forms of the strawberry. I very soon, however, came to the conclusion on looking over it that it was a very remarkable production indeed, and in a scientific sense at least a century in advance of its time.

Duchesne's book is in fact the record of a purely biological study of a small group of plants. The significance of work of this sort has only been thoroughly recognised since the publication of the "Origin of Species." Just as with C. K. Sprengel, whose book was also written in the last century (1793), the world has had to roll on far into another hundred years before it was ready to do justice to this kind of research. There is a curious incongruousness between the freshness and modernness of the ideas and the faded type and musty paper in which they are embalmed.

Duchesne plunges at once into the business of his book in the first line of the preface with a straightforward simplicity not unworthy of Mr. Darwin. I will attempt a translation of the first paragraph:—

"The wish to see if it were possible to raise from seed a plant which scarcely ever produces any has led me by a happy chance to the production of a new race, which made its appearance at Versailles in 1761. This circumstance induced me to more closely devote myself to the study of strawberries, and led me to another discovery. I found that they are not all truly hermaphrodite; forms exist, in fact, which are sexually differentiated.<sup>1</sup> And I have succeeded in the past year, 1765, in fertilising, by means of one set of plants, individuals of another sort, which are cultivated as a matter of curiosity, and are constantly sterile. One, amongst others, has produced fruits of great beauty; M. le Marquis de Marigny has obtained for me the honour of having this submitted to the king, and it is to be raised in the Versailles Gardens by my method. This unexpected success has still more redoubled my ardour to make further observations."

The race so produced, which Duchesne called *Le Fraisier de Versailles*, or *Fragaria monophylla*, is un-

<sup>1</sup> This must be one of the first observations of the tendency of plants with hermaphrodite flowers to pass into the dioecious state. The fact is now well established. (See Darwin's "Forms of Flowers," pp. 278-309.)

doubtedly a very curious plant. All its leaves are permanently unifoliate; i.e. instead of bearing three leaflets, as is ordinarily the case with strawberries, the petioles bear but one. Duchesne observes that this is also the case with the first leaves of all seedling strawberries. *Fragaria monophylla* may be therefore regarded as a form which always retains the juvenile, and never arrives at the adult, foliage, and this peculiarity remains constant in subsequent generations. The effect of crossing, as a potent stimulus to variation, could not but have powerfully impressed Duchesne in so striking a case as this, and further observations seemed to have led him to account for the common characters which otherwise diverging forms exhibited as best accounted for by a common ancestral origin. The study of geographically separated species, however, necessarily led him to see that something more than crossing was needed to account for variation in every case. In discussing *Fragaria virginiana*, a native of North America, which is the origin of the race of Scarlets, Duchesne speculates as to its derivation from the wild *F. vesca* of Europe, and attributes the divergences from this type to the effect of North American soil and climate.

His work on Strawberries, where he was dealing mainly with races, led him to speculate with regard to the higher groups of species, genera, and orders. His results seem to me, for the time, so extraordinarily bold, and therefore historically so interesting, that I quote the first portion of the Recapitulation, pp. 219-221, entire, in the original French:—

"J'ai déjà dit, à l'occasion du Fraisier-ananas, qu'il étoit très-difficile de ranger en ligne droite les diverses Races d'une même Espèce, de manière qu'on pût passer de l'une à l'autre par gradations de nuance. Cela est peut-être aussi impossible, que de ranger en ligne droite les Espèces, les Genres, et les Familles; par la raison que chaque Race, comme chaque Espèce, chaque Genre, ou chaque Famille, a des rapports de ressemblance avec plusieurs autres.

"L'ordre Généalogique est donc le seul que la nature indique, le seul qui satisfasse pleinement l'esprit; tout autre est arbitraire et vide d'idées. J'ai eu soin, à chacune des Races de Fraisiers, d'indiquer ce qui m'a paru vraisemblable à cet égard; mais je n'ose me flatter d'avoir toujours rencontré juste. Il faudroit, pour le bien faire, avoir des connaissances certaines et précises du pays natal de chaque Fraisier, ou bien, du tems où il a été élevé de graine, et de quel autre Fraisier provenoit cette graine; j'ai fait voir combien on manquoit encore de lumières sur tout cela.

"C'est par cette raison que je me suis permis de donner mes conjectures; en voici les résultats; la forme d'Arbre généalogique les rendra encore plus sensibles, et en fera mieux saisir l'ensemble."

It is certainly startling to come upon a phylogeny of the most modern type in a book more than a century old.

It was not till after I had gratified myself with a study of Duchesne's remarkable speculations that it flashed across my mind that attention had already recently been called to them; and I found, in fact, that Prof. Alphonse de Candolle, in a short paper put together with the felicitous erudition of which he seems to possess so inexhaustible a store, had already, in May of last year,<sup>1</sup> stated most of the points on which I have dwelt above. And he mentions that, on the occasion of a visit to Mr. Darwin in 1880 he told him of the existence of the book, which he describes, justly enough, as "a very curious work, older than that of Lamarck, but to which no one had ever referred except for points of secondary interest."

I know little about Duchesne himself. De Candolle says that he was a horticulturist and Professor of Natural History, and that his knowledge was as varied as it was

<sup>1</sup> "Darwin considéré au point de vue des causes de son succès," &c., *Archives des Sciences*, May, 1882.